

Aug. 24, 1955

Dear Josh,

Unfortunately my reply will have to be, if anything, even more tentative than your inquiry. I am not yet so set in my ways as to forego all interest in medical genetics, but the question of availability can be answered only conditionally. The army is still after me and I am now on a years deferment instead of the usual six months. I have been offered Werner Braun's job and am going to look it over at Camp Detrick in a few days. This does not provide an immediate solution to the army problem, but Housewright claims that after I have been established there a couple of years or so the chance is excellent of being assigned to the same post with a commission in the chemical corps. The job is attractive from the salary angle and because of its nearness to civilization. Also he guarantees that I would be able to select several new members of the genetics group, since they are planning an expansion. The obligations to the rest of the MB division would be minimal, with plenty of opportunity for independent research. You can see that my problem in deciding about this is somewhat similar to yours in instigating a department of Medical Genetics; that is, I would feel better about it if I were sure of getting some good people to go with me. I have spoken with Ravin, and he is conditionally interested; he would take the job if conditions should become less hospitable at Rochester, as by Cooper leaving. Housewright has already hired Belser. If it still looks good after my visit, I plan to make inquiries of Norton Zigler and Larry Morse. I have no idea of their availability, but this would be an important factor in making up my mind about the job.

As to your idea for a new department, it is too bad that the key to the situation is to propose the right man, because, conversely, the key to getting the right man is the existence of the department. At any rate you can see the uncertainties involved in my availability for the next few years.

What is this Wollman stuff, anyway? It seems superficially plausible, but gives me an uneasy feeling that they are due for another surprise when the correct explanation is found. I wonder if it could be somehow related to the changes in T/P with time illustrated in Norton's talk here in 1954. The paper omits mention of the frequencies of the recombinant types involving the unselected markers, and gives only the separate frequencies; why? Maybe it didn't seem important because they were dazzled by the vision of an oriented chromosome moving like a worm from one conjugant into the other. More likely than the T/P idea, the effect might be on post-zygotic elimination or on the opportunity for crossing over before such elimination. It seems to me the experiment does not settle the mechanism, but raises further questions.

Things are fairly dull in my laboratory; I'll write again when anything new turns up.

Sincerely,

Kim